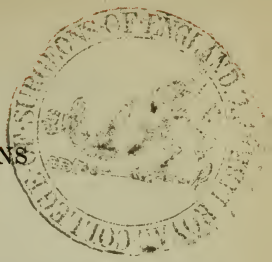

OBSERVATIONS

ON THE

PHYSIOLOGY AND PATHOLOGY

OF THE

NERVOUS SYSTEM:

Retrospective Address

DELIVERED AT THE

TWENTY-SEVENTH ANNUAL MEETING OF THE BRITISH MEDICAL ASSOCIATION IN LIVERPOOL, 1859.

BY

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OBSERVATIONS ON THE NERVOUS SYSTEM.

Mr. President and Gentlemen,—When the committee of your Association did me the honour to request me to deliver an Address in Physiology at this meeting, I acceded to their wish; not, I must confess, without some feelings of my own incompetence, and with a strong conviction of the difficulties of the task I undertook. When I began to reflect on the subject, it seemed to present difficulties of no ordinary character, from the great abundance of material it afforded. I felt that to attempt to generalize on the science of physiology, in the short space of time allotted to an address, would tax powers far greater than mine; and that to endeavour to sketch the recent advances which the various cultivators of the science, in its different departments, had made, would necessarily be attended with failure.

It is true, that some of the great principles which regulate the phenomena of life admit of easy definition, and great and varied application; and no more interesting and important subject could possibly be chosen as the groundwork of an address: but yet, when we come to deal with many of the questions of the science, we often find ourselves rapidly passing from the domain of well grounded theory and recognised laws to that of hypothesis; and from our incomplete knowledge of many of the manifestations of vital action, we are compelled to rest satisfied with but an imperfect explanation of the facts we witness.

But, in making this remark, it is impossible not to rejoice at the high position which physiology now holds as a science, and at the great advances and brilliant discoveries which have marke

its progress during a period comparatively recent; discoveries which have almost revolutionized the whole science of organic life—which have thrown a flood of light on the highly complex phenomena of vitality, and reduced many of them to the utmost simplicity; discoveries which have resulted in generalizations of an application so universal, and an importance so vast, as to entitle them to no lower position as established truths than that accorded to the doctrines of physical or chemical science; for what are the generalizations in physics, or what the laws of chemistry, which have a wider range, or an application more universal, than the principles of nervous action enunciated by the genius of Bell, or of cell-formation, mainly elucidated by the labours of Schwann?

To attempt to sketch the recent progress of physiology in all its varied departments would be alike impossible with an attempt at generalization, so great have been the labours of the cultivators of the science, so numerous are the details of their observations.

Unwilling, therefore, to venture on the responsible task of addressing you on the general principles of the science, and feeling the impossibility of reviewing the whole mass of the subject, I resolved to deal with it in one of its details; a determination to which I the more readily arrived, that I felt that, in addressing those who are engaged in the practical duties of their profession, I should best discharge my functions by giving the subject, as far as I was able, as practical a bearing as possible.

Following, therefore, the example of others who have delivered addresses on kindred subjects, I have felt that in selecting some topic to which more especially to direct your attention, I should be acting quite in accordance with the wishes of my audience, and with the requirements of the subject itself. Indeed, such a course is rendered absolutely necessary at the present day, for to whatever branch of science we direct our attention, we find its sub-divisions so numerous and important, that each one claims a special share of our attention.

But when, after much careful consideration, I determined to choose some special subject as the chief topic of this address, and had flattered myself that I had surmounted an important obstacle, I experienced no small difficulty, and no little trouble, in deciding which branch of the science I should select—on

what series of phenomena I should venture to offer you a few observations.

On considering the various subjects which have engaged the attention of physiologists in recent years, it appeared to me that there was one which, from its importance, the labours devoted to it, and the great advances made in it, possessed undisputed claims to consideration; and, as I am not aware that it has ever been made the subject of a special address in connection with this Association, I have felt that it could not fairly be passed over.

There are no departments of physiology which possess greater interest, there are none in which there have been greater labours, as assuredly there are none which present greater difficulties, than those which relate to the investigation of the functions of the *Nervous System*; and undoubted as have been the advances which have taken place in our knowledge of the actions of the various portions of the nervous centres, during the present century especially, yet that knowledge, we are compelled to confess, is still in many respects imperfect and incomplete; and there is no subject in the whole range of organic science which opens a wider field for research, no one which promises a more abundant harvest with reference to the principles of diagnosis and treatment of disease, than that which relates to the phenomena of nervous action.

My object, to-day, will be to lay before you some observations on the functions of some portions of the nervous centres, and especially to direct your attention to the more recent and very important facts, which experimental physiology and pathological observation have brought to light with reference to the properties of the spinal cord.

The investigation of the properties of the nervous centres is undoubtedly surrounded with immense difficulty. That important clue to function which the knowledge of the minute structure of organs affords, has been hitherto in great measure denied us with reference to the nervous centres. The peculiar nature of the nervous tissue has prevented us from ascertaining exactly how certain portions are disposed of; and although some researches, to which I shall have to refer, have thrown great light on questions of this kind, and have received confirmation from subsequent physiological observations, yet there is still wanting that absolute anatomical proof, without which, as a

rule, no safe theory can be built up with reference to function. But were our knowledge of the structure and arrangement of the nervous system perfect; did we know the exact mechanism by which the phenomena of nervous action are performed; were we acquainted with the processes by which nervous power is generated; or of the nature of that subtle fluid which conveys the mandates of the will, and carries impressions to the sensorial centres—there would yet remain endless subjects for inquiry; a vast mass of facts in connection with that system in which mind is so intimately blended with matter, which is the organ of the intellectual faculties, the source of man's supremacy over the rest of the organic world.

It would be interesting, did time permit, to dwell on the views which were entertained by the ancients with respect to the subject we are considering. These views, however, almost entirely speculative in their character, have but little practical bearing. It was long an opinion, which seems to have been promulgated by Aristotle, that the brain was of no use except as a refrigerator of the hot spirits ascending from the heart, whilst we find that Plato, Galen, and, in a later age, Vesalius, considered it as the seat of the rational faculty of the mind. At an early period, a glimmering of the truth seems to have existed with reference to the functions of the different nerves; for we find Erasistratus teaching that the sensory and motor nerves had different origins; the former arising from the meninges, the latter from the cerebrum; whilst Galen, who performed some interesting experiments on the spinal cord, experiments which have been recently repeated with the most important results, considered that the soft nerves were nerves of sensation, and the hard, nerves of motion. Galen opposed the views of Aristotle with reference to the functions of the brain, the ventricles of which he considered were for the purpose of secreting the animal spirits, to be then diffused throughout the substance of the brain, and from thence transmitted to the nerves; and this opinion, with some modifications, was long entertained. By the Arabs, one of the ventricles was supposed to be the seat of sensation; another, of the imaginative faculty; a third, of the understanding; and a fourth, of memory; and Benevenius, in confirmation of this doctrine, relates the case of a certain thief, often caught stealing, who never could recollect his

previous offences, and who was found after death to have no cerebellum.

Numerous authors, who flourished after the revival of learning, maintained more or less fully the opinions of Galen. Descartes asserted that the animal spirits were secreted from the brain through pores opening into the ventricles, and that when accumulated there they excited the soul through the pineal gland, from which they were distributed to the body. Caspar Bauhin was amongst the first who taught that the animal spirits were generated in the substance of the brain, and from thence were distributed to the nerves, without being collected in the ventricles, and in this view he was supported by Caspar Hoffman. These authors also taught that the ventricles were excretory organs, and their secretion was supposed to find its way through the cribriform plate into the nostrils and fauces, and thus to produce the mucus of those cavities; an opinion which it was reserved for the anatomical researches of Schneider to refute. A view which later prevailed, and which was supported by our illustrious Willis, by Malpighi, and others, was that the cortical substance of the brain was the generator of the animal spirits; a doctrine analogous to that which is conformable with all we know at the present day, with reference to the grey matter of the nervous centres as the generator of nervous power.

But I feel that I must pass from these considerations to more practical points, and more recent observations in connection with my subject.

Valuable as may have been the opinions, and true as may be the theories, of some of the distinguished physiologists who flourished during the eighteenth century, especially with regard to the functions of the spinal cord, a point on which I shall have briefly to dwell, it may be safely said that with reference to our knowledge of the functions of the roots of the spinal nerves, we owe everything to the labours of more recent investigators. Before the time of Sir Charles Bell, all was hypothetical and vague. His discovery at once placed the science of neurology on a firm and stable basis; its importance, its value, the influence it has exercised on subsequent investigations, it is impossible to overestimate. A type of all great discoveries, the result of careful observation and profound reasoning, it stands preeminent amongst the results of the labours of

the present age, and marks an epoch in the history of physiological science.

It would be pleasing to dwell in the language of eulogy on the merits of him to whom science is so much indebted, and briefly to sketch the progressive steps by which he was led to his important results; to show how he saw with his mental eye the structure of those cords, the function of which he had discovered; and how, whilst unable to see that which the microscope has since demonstrated—the isolation of each nervous fibril—he yet was led to appreciate accurately the important fact—but time forbids, and I pass to other topics.

There is no subject which presents peculiarities of greater interest, there is, perhaps, no one which has led to greater controversy, in connection with the nervous system, than that which relates to the reflex, or automatic, movements of the body; and whilst it will be admitted by all that to the labours of one of our most distinguished countrymen we are mainly indebted for a more definite and accurate knowledge of these actions than formerly existed, yet we feel that justice would not be done to others, did we pass over in silence the views and the labours of physiologists of a past century, who seem to have appreciated in a very remarkable manner the nature of the phenomena to which we are alluding.

Those who have read the works of Whytt, of Unzer, and of Prochaska, cannot but have been convinced that those authors had formed very accurate views of the various involuntary movements of the body; and, although some discussion has taken place as to the exact meaning of a term used by Whytt, yet there can be but little doubt that he appreciated the fact, that an impression made on the periphery of a nerve might be carried to the nervous centre, and there, without producing sensation, give rise to a reflex action. But Unzer has enunciated, in a much more complete manner than Whytt, the doctrines of reflex action; and has clearly pointed out the distinction between acts resulting from impressions only, and true psychical actions derived from the brain. He says motions may be produced as the result of external impressions, although these impressions cannot reach the brain, even after an animal has been decapitated, so that no sensations can be felt. He calls them "nerve actions," in contradistinction to those which are the result of consciousness, which he terms "soul actions." I cannot give

the quotations from Unzer's work which shew clearly his views, but he distinctly enunciated the principle, that the nervous system is not simply destined to propagate external impressions to the brain, there to produce sensation with consciousness; but that it possesses also the distinct property of giving rise to movements as the result of an external impression, which does not reach the organ of consciousness; and, in an answer to one of his reviewers, he distinctly expresses his belief in the reflex function of the spinal cord, for he says that, as nerves connected with ganglia may experience from external stimuli a *moving power*, a reflection of impressions upon other fibres which are regularly determined to action, "*just so is it with the nerves which arise in the spinal cord, and have there probably the double function: partly to transmit these impressions to the brain, that sensations may be excited, and partly, to be reflected on other nerves, so that thereby certain movements are developed which otherwise would not result from these impressions.*"

It is impossible to enter fully into the views of Unzer, many of which have received confirmation at the hands of physiologists of recent date; but I must not pass over the works of one of his successors, Prochaska, who flourished at the close of the last and the beginning of the present century, and who more accurately defined than previous physiologists the portion of the nervous centres from which reflex actions are derived. After stating that external impressions made on sensorial nerves are transmitted to the origin of the nerves, and are thence reflected on to corresponding motor nerves, he says that the part where these sensorial and motor nerves meet, *and whence motor actions are reflected*, is called the *sensorium commune*. This sensorium commune consists, not of the cerebrum proper and cerebellum, but extends through the medulla spinalis, the medulla oblongata, the crura of the cerebrum and cerebellum, and part of the thalami optici; in a word, he says it is co-extensive with the origin of the nerves. He was fully aware that reflex actions might take place independently of consciousness, and he terms them automatic actions. He is also, I believe, the first author who mentions that these reflex actions take place in obedience to a law which regulates the preservation of the individual.

These observations are sufficient to show, that the various phenomena of reflex action were well known to the physio-

logists I have alluded to, and that they had analysed their relations to the nervous system with great sagacity.

It has been a common impression that, up to the time of Marshall Hall, these various so-called sympathetic actions were regarded by physiologists as dependent on sensation; for, to use the words of one of the writers in the *British and Foreign Medical Review* for 1837, "The phenomena called excito-motory are precisely those which come under the old denomination of sympathetic; and which we believe have generally been thought to be dependent on sensation." Now it would not be difficult to show that, although the views of Unzer and Prochaska seem to have been but little received, there is yet abundant evidence that all physiologists did not consider these movements as dependent on sensation. Time does not permit that I should refer to the passages in the writings of such authors, in proof of this statement; but the works of Hunter, of Sir Gilbert Blane, of Mayo, of Flourens, will furnish abundant instances.

But notwithstanding this, it must be admitted that a very imperfect notion generally prevailed of the nature of these automatic movements; and Dr. Marshall Hall was undoubtedly the first to revive the doctrines of reflex action, to establish, with greater precision than his predecessors, the laws by which its phenomena are governed, and to assign to them their proper position in physiological science. Although he was to a certain extent anticipated in some of the essential parts of his system, yet he was the first to apply that system to many important pathological phenomena, and to the elucidation of various forms of nervous affections. The physiology and pathology of the nervous system received a new impulse from his researches. His whole life was devoted to the study and investigation of the reflex or excito-motor system, which he pursued with all the perseverance and ardour which characterize the discoverer, fully convinced of the importance of his doctrines. He worked incessantly to illustrate his principles, to simplify and enlarge the basis on which they were founded; and whatever may have been the deficiencies or the imperfections of his system, the prominence he gave to his views, by the number and scientific character of his writings, and his own indefatigable exertions, had the effect of directing the attention of observers in all parts of the world to

the facts of which he treated, and of establishing, as a general principle of science, that which had been previously admitted only by a few.

The special views of this distinguished physiologist, that there is a distinct set of excitor nerves and motor nerves belonging to the reflex system, independently of the ordinary sensory and voluntary nerves, as yet rests on no absolute anatomical proof, and has never been universally admitted as true. That each portion of the spinal marrow is an independent centre of nervous action, admits of indubitable proof; and that the cord is the channel of communication with the organ of consciousness, I need scarcely mention. The exact mechanism by which the automatic movements are accomplished, and by which sensitive impressions are conveyed to the brain, and motor impulses to the muscles, yet requires elucidation; but there are some facts which examination of structure, and experimental investigation, have brought to light, that have important bearings on this point, and on some of these I propose briefly to dwell.

By most anatomists, before the time of Gall and Spurzheim, the spinal cord was considered as a mere bundle of nerves, a prolongation from the brain, but those authors pointed out the untenable nature of this view. The existence of the cord in acephalous fœtuses, its size in proportion to the number of nerves derived from it, and the direction of the fibres of the nerves, led them to maintain this opinion. They further stated that the nerves arose from the grey matter of the cord, but the statement seems to have been founded on no anatomical proof. The first anatomist in this country, who appears to have investigated the relation of the roots of the nerves with the spinal cord, was Mr. Grainger; and his observations showed that these roots were in part connected with the grey matter, and he was led to believe that they were in part continuous with the longitudinal fibres of the cord, and thus passed on to the encephalon. The observations of Mr. Grainger were somewhat anticipated by those of Bellingeri, who had previously traced the anterior roots to the grey matter in the ox.

Since the time when Grainger made his observations, numerous anatomists have directed their attention to the structure of the spinal cord and the arrangement of the roots of the nerves; and, by employing various processes of preparation,

have been able to make thin sections of the cord, such as they could examine with tolerably high powers under the microscope. Amongst the most important observations that have been made are those of Volkmann, Kölliker, Stilling, Lockhart Clarke, Schilling, Gratiolet, and most recently of all, Lenhossék and Schroeder Van der Kolk. Considerable discrepancy exists between the statements of these authors. On the one hand, we find Volkmann asserting that the longitudinal columns of the cord undergo no progressive increase of size from below upwards. Now this is a very important point; for if the sensory and voluntary nerves are derived from the encephalon, and pass off from the cord in the various spinal nerves, the cord must contain a much larger number of fibres at its upper than at its lower part, and we should expect that the columns of the cord would gradually increase in size from below upwards. Volkmann further maintains that the quantity of white matter is everywhere in proportion to the quantity of grey; his observations were made on the spinal cord of the horse, and have been contradicted by Kölliker, who asserts that in man the thickness of the white columns does augment from below upwards, and that the increase in the diameter of the cord, at the ganglionic enlargements, is due to the augmentation of grey matter only. He, moreover, states that the nerve-tubes of the cord, especially in the upper part, are much smaller than those of the nerve-roots; and he asserts from actual measurement, that it is by no means impossible for the fibrous strands of the cord to contain all the nerve-tubes of the spinal nerves.

The researches of Mr. Newport into the structure of the articulata, seemed to establish a very important fact in connection with the nervous system of those animals. He stated that he had found that certain of the fibres of the nerves proceeding to the analogue of the spinal cord, passed onwards to the cephalic ganglia, whilst others passed into the ganglia of the cord.

The theory deduced from Newport's view of the structure of the nervous system, is that the nerves which proceed to the cephalic ganglia, are the agents by which sensations are conveyed to the latter, and in respondence, these ganglia direct and harmonise the general movements of the body; whilst the nerves which are simply connected with the ventral ganglia are the agents of the excito-motor actions.

This view of the structure and functions of the spinal cord has been adopted by Dr. Carpenter, who considers that the cephalic ganglia of the invertebrata are analogous to those ganglia which are found at the base of the brain in vertebrata, and which he believes to be the centres of sensation and instinctive motor action, whilst each portion of the spinal cord ministers to the automatic movements of the body. Dr. Carpenter endeavours to reconcile the structural views of Kölliker and Volkmann, on the principle that it is not at all improbable, that different animals should have different proportions of nerve-fibres terminating in their automatic and sensorial centres, according as their movements are more or less analogous to those of the invertebrata, or more or less performed in obedience to the influence of the will, and under the guidance of sensation.

Notwithstanding the high authority on which these views rest, they are not conformable with the results obtained by the observations of other investigators. From microscopical examinations of the disposition of the roots of the nerves, there can be no doubt that many of the fibres terminate in the grey matter of the cord, at or near the spot where they join it. Lockhart Clarke has shown that the posterior roots, on entering the cord, are disposed of in three ways; some of their fibres pass transversely, and enter the grey substance; others pass downwards for a short distance, and then terminate in a similar manner; whilst a third set pass obliquely upwards, but he has not been able to trace any of them continuous with the longitudinal columns of the cord, and the inference therefore is that, after a longer or shorter course, they enter the grey substance. This view of the disposition of the fibres of these roots is borne out in a remarkable manner by experiments on living animals. With regard to the anterior roots, they have been traced to the anterior cornua, and they are said to take their origin from ganglion cells, which exist in groups along the cornua. This arrangement of the anterior roots has been most recently pointed out by Schroeder Van der Kolk, who does not admit that any of their fibres are directly continued on to the brain; he differs, however, from Lockhart Clarke and others with regard to the fibres of the posterior roots, which he says consist in part of sensitive nerves, and in part of excitor nerves; the former passing up in the posterior columns to the ence-

phalon, whilst the latter are connected with the grey matter of the cord. In support of this view, he states that the anterior columns do not increase in size from below upwards, but that the posterior columns progressively enlarge.

These views of Schroeder Van der Kolk, and the physiological inferences he deduces from them, are opposed, in a very important manner, to the results of experimental investigation into the functions of the columns of the cord, to a brief consideration of which I now pass.

Before the time of Sir Charles Bell, when the difference in function of the two roots of the spinal nerves was unknown, no attempt had been made to assign different functions to different columns of the cord. Sir C. Bell first promulgated the view that the posterior columns, like the posterior roots of the nerves, were the conductors of sensitive impressions, whilst the antero-lateral were the channels of motor influence; he, however, gave no further support to his theory than the following very incomplete experiment:—"I found," he says, "that injury to the anterior portions of the spinal marrow convulsed the animal more certainly than injury to the posterior portion; but I found it difficult to make the experiment without injuring both portions."

This theory of Bell, which I may here state was subsequently in great measure abandoned by him, was opposed by Bellingeri, Schœps, Rolando, Calmeil, Nasse, and others; but was strongly supported by an eminent French physiologist, M. Longet, who attempted to prove that it was as correct as that respecting the functions of the roots of the nerves.

As the views of M. Longet have been extensively received as true, both by physiologists in this country and on the Continent, and as they seem to me, especially with reference to the channels for the conduction of sensitive impressions, to have received an entire refutation at the hands of recent investigators, I feel it my duty to consider them somewhat in detail.

M. Longet is of opinion that all the sensitive nerve-fibres of the spinal nerves enter the posterior columns of the cord, and go up to the brain in these columns, and that therefore sensitive impressions are only received from these parts of the cord and their prolongations in the encephalon:

That the restiform bodies of the medulla oblongata, being the continuations of the posterior columns of the cord, are

also the only channels for the transmission of sensitive impressions:

That the sensitive impressions, going to the sensorium, have to pass chiefly across the cerebellum, as the restiform bodies chiefly pass across that organ.

Dr. Todd, in this country, raised the first serious objection to this view. He pointed out, on anatomical grounds, the improbability of the theory, and detailed some pathological facts which tended to negative it.

With the exception of the researches of Schroeder Van der Kolk, recent anatomical evidence is opposed to the theory; but the most important evidence which has yet been given with reference to the question, is to be found in the results of the experimental observations and in the pathological facts detailed by Dr. Brown-Séquard.

The experiments of Dr. Brown-Séquard were made on some of the lower mammalia. They consist in laying bare a portion of the spinal cord by removing the arches of the vertebræ, and in dividing different portions of the cord. We are assured by the experimenter that the objections which have been raised against his views, on the ground that no satisfactory results can be obtained after the spinal canal is opened, are altogether untenable; for, "When the operation is done quickly, even if a considerable part of the cord be laid bare, if hæmorrhage have not been great, and if pain have been avoided by the exhibition of chloroform, there is no notable diminution of sensibility, and there is no diminution in the voluntary movements, except that depending upon the section of the muscles of the back."

The first fact pointed out by Dr. Brown-Séquard to which I shall refer, is that a transversal section of the posterior columns of the cord does not produce a loss, nor even a diminution, of sensibility; on the contrary, that it is followed by an increase in the degree of that property. The section, therefore, of the supposed only channels of transmission of sensitive impressions results, not in anæsthesia, but in hyperæsthesia in all the parts supplied with nerves behind the seat of section. Further, it appears that a complete transverse section of the restiform bodies is followed by an increase of sensibility in every part of the limbs and trunk; and that the same results, but in a less degree, follow a transversal incision into the cerebellum, the

processus cerebelli ad testes, and the *tubercula quadrigemina;* and that if the restiform bodies be dissected from their neighbouring parts and removed, the animal operated on becomes hyperæsthetic.

These are assuredly very important facts, and they afford convincing proof, at any rate, that the posterior columns of the cord, and the restiform bodies, are not the *only* channels by which sensitive impressions are conveyed from the posterior roots of the nerves to the encephalon; but other experiments tend to show that these parts are in no way concerned in the function at all.

After the experiment I have detailed, the natural step to take is to ascertain the result of division of all the parts of the cord except the posterior columns. Such an experiment has been frequently performed. Without mentioning the results obtained by Stilling, Schiff, and others, I may state that Brown-Séquard has found that when the whole of the spinal cord, except the two posterior columns, is divided transversely, say, for instance, in the dorsal region, the posterior limbs become deprived of sensibility. This experiment affords another proof that the posterior columns are not the channels for the transmission of sensitive impressions to the brain, and other observations seem to show that the lateral columns of the cord have no part in the function.

A variety of experiments it is unnecessary to detail, which consist in dividing every part of the spinal cord except the central grey matter, tend to prove that this central grey matter is the principal conductor of sensitive impressions to the brain; but there are some facts which indicate that it is not the only portion of the cord thus employed, and that the anterior columns have a certain share in the function, for it has been found that, when the whole of the grey matter is divided, the anterior columns being left, sensibility, which disappears immediately after the operation, after a time returns to a slight extent.

A very important question arises, as to what part of the grey matter is the conductor of these sensitive impressions. Experiments seem to show that the central portion, that connecting the cornua together, is the most concerned in the function. We learn from the researches of Lockhart Clarke, Schroeder Van der Kolk, and others, that the grey matter consists in part

of ganglion-cells, and in part of nerve-fibres, the latter of which form longitudinal columns, surrounded by grey substance. No proof, however, exists that they pass continuously up to the brain, and Lenhossék maintains that they are communication-fibres between different ganglion-cells. If any of the fibres are continued on to the brain, they probably take part in the function of transmitting sensitive impressions. Although the posterior columns of the cord appear to have no share in transmitting sensitive impressions to the encephalon, yet there is proof that the fibres of the posterior roots pass through them to enter the grey substance; for, when a transverse section of these columns is made, the nerve-roots attached to the inferior segment are highly sensitive, a fact which seems to be due to some of the fibres of those roots passing downwards to reach the grey matter. This experimental observation entirely accords with Lockhart Clarke's anatomical researches previously alluded to.

The general conclusions which may be deduced from experiments and observations which I cannot detail, are that the conductors of sensitive impressions, when they reach the spinal cord, either at once terminate in the grey matter, or else, passing obliquely for a short distance, some upwards, and some downwards, ultimately terminate therein.

Another important question, which seems not to have occupied much of the attention of physiologists, but which appears entirely set at rest by recent experiments, is that which relates to the decussation of the conductors of sensitive impressions in the spinal cord.

It was the universally received opinion that no such decussation took place, and therefore that the sensitive impressions must cross in the encephalon. Sir Charles Bell tried to discover the seat of decussation, and he imagined it to be above or near the crossing of the pyramids; but he gave no proof in support of his idea.

The section of a lateral half of the spinal cord is the experiment most calculated to give us proof of the decussation, or otherwise; and such an experiment has been frequently performed; but unfortunately no observations, until recently, were made as to its effects on the sensibility of parts supplied with nerves behind the section. Schœps Van Deen and Stilling found that sensibility was not destroyed on the side of section;

but they did not recognise the condition of the opposite side.

We are indebted to Brown-Séquard for the solution of the question we are now considering, and for the discovery of the fact that there is a decussation in the spinal cord of the conductors of sensitive impressions. The following are the results of his observations. He has found in mammals that, when a section of a lateral half of the cord, including the posterior, the lateral, and the anterior columns, with all the grey matter of one side, is made, sensibility seems much increased on the side of section, and much diminished or entirely lost on the opposite side. In fact, there is hyperæsthesia on the side on which the division is made, anæsthesia on the other.

But another experiment may be performed to prove that decussation takes place. It is one which was performed by Galen, and was repeated by Flourens; but its effects on sensibility were entirely overlooked by both. It consists in making a careful division of the cord—say of the lumbar portion—into two lateral halves. If such an experiment be well performed, only the commissures of the cord are divided, and all the longitudinal elements are left uninjured. A striking result is then obtained: the voluntary movements still exist in the posterior limbs, but sensibility is entirely lost in them. This loss of sensibility must depend on the section of the sensitive conductors, as they cross the median plan of the cord.

From a vast number and great variety of experiments, to the details of which I must refer all who desire to investigate this subject, it appears that we may consider it as an established fact, that the conductors of sensitive impressions make their decussation in the spinal cord, at a short distance from the point of insertion of the posterior roots of the nerves. Experimental investigation seems to show that this decussation is complete in the lower mammalia, and pathological facts appear to confirm the view with regard to man.

There are certain pathological phenomena of great importance, hitherto difficult of explanation, on which much light has been thrown by recent experiments. It is well known that considerable alteration of the spinal cord may exist without loss of sensibility ensuing. It appears that, when the spinal cord is divided transversely, and the section reaches the grey matter and divides a portion of it, sensibility becomes dimi-

nished in every part of the body behind the seat of section, and not more so in one than in another; and a partial section of one side is followed by diminution of sensibility on the opposite. From these results, it would appear that, however small be the portion of grey matter remaining, it is capable of transmitting a certain amount of sensation from all parts of the body behind it. We here have an explanation of the fact so often seen in cases of disease of the spinal cord—a persistence of sensibility, when, after death, we find a large portion of the cord destroyed.

From the consideration of the parts in the spinal cord, which are the agents for transmitting the sensitive impressions to the brain, we naturally pass to those concerned in the transmission of the orders of the will to muscles.

The theory of Sir Charles Bell, that the anterior columns were destined for this function, rests on no proof afforded by that distinguished physiologist; but many experimenters have stated that a section of these columns produces paralysis. Stilling has, however, denied this. The experiments of Brown-Séquard tend to show that a section of the posterior columns does not affect the voluntary movements, but that a division of the whole of the cord except the posterior columns produces complete paralysis. He further states, that a section of the posterior half of the cord, including the whole of the central grey matter, is followed by an almost total annihilation of voluntary movement; and if the anterior horns of the grey matter be divided as well, the loss of power seems complete. This would lead us to infer, that the anterior columns have but little or nothing to do in the transmission of the orders of the will to muscles; but then, on the other hand, it is found that a section of these columns alone is followed by an almost entire loss of power. From these observations, we are forced to the conclusion that all parts of the cord, except the posterior columns, are more or less concerned in transmitting the orders of the will; but this remark does not apply to the upper part of the cord, for it appears that, near the medulla oblongata, most of the voluntary conductors pass in the lateral columns and the grey matter between the lateral and anterior columns.

The results I have mentioned, as following the section of the anterior columns of the cord, in some measure agree with the views, founded an anatomical grounds, of Schroeder Van der

Kolk, who is of opinion that the fibres of these columns terminate in the ganglion-cells of the grey matter, from which the motor nerves arise; and that they transmit the influence of the will to the ganglion-cells, which then act on the nerve-fibres.

If we compare the results obtained by vivisections of the lower animals with those which pathological changes in the spinal cord have produced in man, we find a remarkable confirmation of the theories which have been advanced. A large and very admirable selection of cases has been made by Brown-Séquard, which tend to throw much light on the functions of different portions of the cord. And first, with regard to the posterior columns, it has been found that, whether these columns are injured by a wound; whether they are submitted to pressure by a tumour or by a piece of bone; or whether their structure is to a slight extent destroyed,—the sensibility of the parts behind the seat of injury remains. Hyperæsthesia even has been known to exist, just as takes place in the lower animals after section of the columns.

These cases also prove that the grey matter is the chief channel for transmission of sensitive impressions. Several cases are recorded in which sensibility was persistent, where *post mortem* examination revealed extensive softening of the white substance, the grey matter being apparently in a normal state; and, on the other hand, cases are mentioned which show that destruction of the grey matter, the white substance being unimpaired, is attended with a diminution or loss of sensation.

Pathological facts afford abundant evidence of the decussation of the sensitive conductors in man. On this point, I shall content myself with remarking, that a careful examination of a considerable number of reported cases appears to prove that such a decussation takes place near the spot at which the fibres enter the cord; and that, having decussated, they pass upwards chiefly in the grey matter to the encephalon. Observation shows that no decussation of sensitive fibres takes place in the pons Varolii; all have decussated before reaching that spot. Disease, or injury, or pressure of the pons, medulla oblongata, or medulla spinalis, produces a loss or diminution of sensation on the side opposite the affection.

I have alluded to the circumstance that experiments seem to show that, at the upper part of the spinal cord, the anterior columns do not transmit the mandates of the will. Anatomy

and pathological facts tend to confirm this. The decussating fibres of the anterior pyramids are almost entirely derived from the lateral columns of the cord; and thus there is no decussation of the anterior columns. Notwithstanding this, an alteration of one side of the medulla oblongata, above the decussation, produces paralysis of the opposite side; whilst an alteration of one side of the cord, below the decussation, produces paralysis of the same side.

Dr. Brown-Séquard has attempted to prove that the various kinds of sensitive impressions have different conductors in the spinal cord; and also that the nerve-fibres employed in transmitting to the cord the sensations of *touch*, *tickling*, *pain*, *heat*, and *cold*, and the sensation which accompanies *muscular contraction*, are as distinct one from the other as they all are from the fibres of the motor nerves. He mentions several cases in which some of the sensations were absent whilst others remained; where, for instance, there was loss of feeling of *tickling* or *contact*, persistence of feeling of *pain;* loss of *tactile sensibility*, of the faculty of feeling *pinching*, *pricking*, and the passage of a *galvanic current*, and persistence of power of feeling *cold* and *tickling*. These certainly are very remarkable cases; but, before we can admit the independence of the nervous fibres conveying these various sensations, very strong evidence is necessary. I would venture to submit, that it is not at all improbable that a single nervous fibre may have the power of transmitting the various kinds of sensations alluded to, depending upon the nature and the power of the stimulus by which it is excited, and the locality on which the stimulus acts.

And here I may, perhaps, be allowed to say a word with regard to those nerves which are supposed to be solely employed as the excitors of reflex action; the existence of which is maintained by Schroeder Van der Kolk on anatomical grounds. The great bulk of evidence which microscopical observations afford us, seems to me to show that the nerve-fibres of the roots of the spinal nerves terminate in the grey matter of the cord, and that none of them are continued on to the brain; and this view, which was originally promulgated by Dr. Todd, appears to receive confirmation from the experiments I have detailed. According to this view, the grey matter of the spinal cord receives the impression from the centripetal nerves, and either carries

it to the encephalon, to produce there a sensation, or, if the communication with the encephalon be cut off from any cause, the impression does not result in sensation, but may give rise to a reflex movement, from the independent action of that part of the spinal cord which received the impression.

With respect to the existence of nerve-fibres for the special function of the excito-motor system, as I have previously stated, no absolute proof exists; and, although it may be somewhat contrary to our notions to believe that a nerve-fibre is capable of performing the double function, of conveying at one time a sensitive impression to the nervous centre, and at another, a stimulus which produces a reflex motor act: yet there seems to me to be nothing improbable in the view, for the impression made is the same, whether it result in a sensation or a motor action; the difference is in the condition of the nervous centres, which, in one instance, allow of its transmission to the sensorium, and in the other do not. The impression made on a centripetal nerve, and received by the cord, would become a sensation were there nothing to prevent its transmission to the brain; but this impediment existing, the impression results in a reflex act, which would have been controlled had the impression been felt.

A view has been advanced by a physiologist, to whose opinion much attention is due, that the spinal cord is not only a centre of motor action, but also of sensation and volition; and that decapitated animals exhibit phenomena which are not explicable except on the supposition that sensations are perceived, and that these give rise to movements which are guided and controlled by the will. This view seems to me to be open to very serious objections; and all the phenomena to which Mr. Lewes has referred in support of his view, appear to me to be simply instances of the excito-motor action of the spinal cord.

Although it is now generally agreed amongst physiologists that the spinal cord is an independent centre of nervous action, and that one part can act without the other; yet it is still maintained by some that the reflex power, which the cord possesses after it has been separated from the encephalon, is the remainder of an influence derived from the encephalon before its separation. In opposition to this view, it has been shown that, in an animal in which the spinal cord is divided, and which has been subsequently killed by cutting the carotid artery, after the

reflex action has ceased, it will return if blood be injected into the body so as to restore the circulation in the cord; thus showing that the reflex faculty is a vital property of the cord, depending on the organisation of the part. The reflex faculty often remains after considerable injury has been done to the spinal cord; for instance, when it has been crushed,—a point of some practical importance as tending to show that, although reflex action may exist after a fracture or luxation of the spine, the cord may be very far from sound.

I now pass to consider briefly some of the results of recent experiments and observations with reference to the functions of the sympathetic nerve, and the influence it exercises over the processes of nutrition and secretion.

As long ago as 1727, Pourfour du Petit had ascertained, that a division of the cervical portion of the sympathetic was followed by contraction of the pupil and increased vascularity of the conjunctiva. Dupuy in 1816, Brachet in 1837, and Reid in 1838, confirmed these observations.

The researches of Claude Bernard, Dr. Augustus Waller, Dr. Budge, Dr. Brown-Séquard, and others, have proved that section of the cervical portion of the sympathetic and galvanization of the same, produce exactly opposite effects. The investigation of the phenomena resulting from experiments on this nerve, promises to throw considerable light on the various processes of healthy and morbid action.

The results of section of the sympathetic in the neck may be briefly summed up as follows:—

Contraction of the pupil; dilatation of the blood-vessels, and increased vascularization, of the parts to which the divided nerve is distributed; an increase in those actions which are considered the result of the vital properties of the parts.

On the other hand, galvanization of the divided nerve is followed by—dilatation of the pupil; contraction of the blood-vessels; diminished vascularity; and a decrease in those actions considered as the result of vital properties.

These results are now established by a vast number of experiments, and are admitted as true by all physiologists.

I may here briefly consider an important question, which has occupied much attention of late years, viz.: What is the origin of the sympathetic nerve? The experiments of Dr. Waller

and of Professor Budge seem to prove that the nerve-fibres of the sympathetic going to the iris originate in the spinal cord, between the sixth cervical and the fourth dorsal vertebræ. Dr. Brown-Séquard states that the origin of these fibres is more extended still, for a section of the lateral half of the spinal cord, at the level of the fifth, sixth, and even the ninth dorsal vertebra, affects the iris in the same way as section of the sympathetic, only to a less degree. And he further mentions, in accordance with the observations of Schiff, that some of the fibres animating the iris ascend the cervical part of the cord, and most likely go up to the medulla oblongata. Observations tend to show that the nerves which supply the blood-vessels are derived, chiefly, if not entirely, from the cerebro-spinal centre. With regard to those which pass to the blood-vessels of the face, as long ago as 1852, Brown-Séquard stated that they arose from the upper part of the spinal cord, and the neighbourhood of the medulla oblongata, and that, after passing down the cord, they emerged with the last cervical and upper dorsal nerves. He has also shown that a section of the lateral half of the cord, in the dorsal region, produces in the lower limbs most of the effects produced in the head by section of the cervical sympathetic, viz.: dilatation of blood-vessels, elevation of temperature, hyperæsthesia. We have here an explanation of the increased sensibility which I have before alluded to, as following section of the white portion of the cord, on the side of section. The increased sensibility is caused by the influence of the operation on the nerves of the blood-vessels.

These results are analogous to those obtained by Mr. Lister, to whose valuable paper on the subject, in the *Philosophical Transactions*, I beg leave to refer my hearers. His experiments were made on frogs; and they afford convincing proof that the cerebro-spinal axis is the nervous centre which regulates the contraction of the blood vessels, and that this function is exercised by the whole length of the cord and the posterior part of the brain, operating through fibres which arise from the axis and pass off with the sensory and motor nerves.

I might relate numerous experiments showing the influence of section of the sympathetic in the neck on the structures of the head and face; but I must refer you to the works of M. Claude Bernard, and others, for the details of them. Nothing is more remarkable than the rapid dilatation of the blood-vessels

and the increase in the temperature of the parts, which take place after such section. The increase in temperature is persistent, and in one animal operated on by Claude Bernard, it was found to exist a year and a half after the operation. Equally remarkable with the results just mentioned is that which follows galvanization, for no sooner is the cephalic segment of the divided sympathetic submitted to the influence of galvanic action, than the increased vascularity of the parts disappears, and the temperature falls below the natural standard.

The effects which follow the section of the sympathetic seem to be in great measure due to paralysis of the blood-vessels; but yet the phenomena are not altogether explicable on this supposition. The increased vascularity, no doubt, results from the dilated condition of the vessels, and the increased temperature may depend on the increased vascularity; but we are told by Claude Bernard, that the vascularity may diminish, without a corresponding diminution of temperature taking place.

The changes produced on the blood-vessels, by a section of the sympathetic, although they resemble, in some measure, those which accompany inflammatory action, yet differ from them in many important respects. No serous, or other effusions occur, unless some accidental circumstance give rise to inflammation, which may then run its ordinary course, and the parts recover their previous condition. There is further, a remarkable power possessed by these parts of resisting the influence of cold; for if an animal, the cervical sympathetic of which has been divided on one side, be placed in a temperature much below that of its own, the temperature of the sound side will sink much more rapidly than that of the injured one; and again, under the depressing influence of slow death, the same phenomena are witnessed; the parts on the side of section preserve their vitality for a longer period than those of the opposite side.

The nerves of blood-vessels are capable of being influenced by indirect, as well as direct, means; and they exhibit, under different circumstances, all the phenomena of reflex action. It has been found, by Drs. Tholozan and Brown-Séquard, that if one hand be dipped into water of a very low temperature, the vessels of the opposite hand will contract; and the more pain is felt the greater will be the contraction. It has also been shown, that excitation of the cutaneous nerves of the neck will

produce a contraction of the blood-vessels of the ear. These effects must be the result of reflex action, and seem to point out the spinal origin of the vaso-motor nerves. It has also been shown that a spasm of the blood-vessels may be produced just in the same way as a spasm of muscles. A section of one-half of the spinal cord, near the medulla oblongata, is followed by the singular result of paralysis of the blood-vessels of the extremities of its own side, and a spasm of those of the opposite side—a spasm which may be so great that circulation becomes almost entirely suspended.

The various experiments which have been performed on the sympathetic nerve, tend to show the influence the spinal portion of it exercises on the functions of organic life. It was long ago shown that the various processes of nutrition, secretion, and repair, might go on in an organ, when the influence it derived from the nervous centres was cut off; and there is very strong evidence that animal life can exist without the endowment of a nervous system.

But in making observations on the sympathetic nerve, we must not forget that it contains another element besides its spinal one, and we have yet to learn the exact nature of the influence which this portion exercises, and the manner in which it is effected by the various processes employed in experimenting on the nerve.

I have alluded to the fact that, by a reflex act, blood-vessels may be made to contract; and it is no less true that, by a similar act, an influence may be produced on the secretion of an organ, and on the nutrition of a part. I need scarcely give instances to prove these points. One or two familiar ones may suffice: the secretion of saliva when the stomach is irritated by food; of gastric juice when the nerves of taste are excited; the production of tears from irritation of the branches of the fifth. These are sufficient to prove the fact with regard to secretion; and of the influence of reflex action on nutrition, abundant evidence exists; but no part affords so good an opportunity of witnessing it as the eye. We know that injury to the branches of the fifth nerve, about the eye, often results in inflammation of the eye itself; and that the inflammation of one eye, especially if it be of a traumatic character, is very liable to produce inflammation of the opposite one. These results must be produced by the irritation being carried to the nervous centres, and thence reflected back.

Again, take the instance of neuralgia. How often does it cause congestion of the eye, and photophobia? M. Notta mentions that of one hundred and twenty-eight cases of neuralgia of the trigeminal nerve, the eye was congested in thirty-four, there was photophobia in eighteen, whilst a real opthalmia existed in some. We must all be familiar with instances of a similar character. Other, and even more striking examples, might be mentioned; but I must pass on. We are all aware that sudden death has sometimes occurred from drinking cold water on a hot day: from a blow on the abdomen; or any severe irritation of the abdominal sympathetic nerve. This seems to result from reflex action arresting the movements of the heart. The influence travels to the spinal cord, and is reflected along the par vagum. That this is the true explanation appears to be proved by the experiments of Brown-Séquard, who states that if the par vagum, or the spinal cord, or the splanchnic nerves, are divided, any kind of irritation may be made on the sympathetic in the abdomen, without an arrest of the heart taking place.

Again, I may instance, as the results of the reflex actions of vaso-motor nerves, the remarkable pathological changes in the internal organs, which so frequently follow extensive burns of the skin; results analogous to that so often produced by irritation of the sensitive nerves of the eye, from a prolonged use of the microscope, an inflammation attacking the eye opposite to the one used.

I might relate other examples, which show the effect of irritation of a centripetal nerve, in producing alterations in distant parts. I mention one other, and that a striking one. An operation is performed on the *cervix uteri*—a polypus is extirpated—and some days after, the actual cautery is applied to the pedicle which remains. Intense peritonitis supervenes, and the patient dies in three days. The autopsy reveals no perforation; a simple reddening of the mucous membrane of the uterus; the Fallopian tubes healthy; the spot cauterized is far from the peritoneal cavity; but yet the intestines are found covered with pus. This case, the details of which are probably familiar to most of my hearers, is a most remarkable and instructive one; not a case of inflammation, reaching the peritoneum by extension; but clearly commencing there. And to what can it be attributed, except to the irritation of the nerves of the *cervix*

uteri being propagated to the spinal cord, and from thence reflected by other nerves to the peritoneum.

The influence of impressions on peripheral nerves in producing changes in the nutrition or secretion of a part, by reflex action, was long ago pointed out by one of our associates, Dr. Radclyffe Hall, in his able work on the sympathetic nerve. He there endeavoured to show that such impressions may produce reflex action of the ganglia, resulting in changes in the organic processes, over which the sympathetic presides; and further, that when the impressions reach the spinal cord, the reaction may produce every variety of spinal phenomena, and influence the organic actions of any or every organ of the body. In the inferences, deduced from his experiments, he has anticipated views of more recent investigators, to which attention of late has been much directed.

I have dwelt somewhat at length on this subject, both on account of its great practical importance, and that I believe the mechanism, by which these results are produced, is not generally admitted to be the same as that by which the reflex action of muscles is produced. And yet the researches of Müller, Stilling, Radclyffe Hall, Claude Bernard, and others, as well as those of Brown-Séquard, seem to leave no doubt of the fact. The latter author has attempted to show, by various experiments, the influence which the spinal cord exercises on these actions.

I have alluded to the internal inflammations which follow large burns, and their supposed production, by reflex action, on the vaso-motor nerves. Dr. Brown-Séquard has found that if an animal, in which the spinal cord has been divided at the level of the third or fourth lumbar vertebra, so that no irritation can be propagated from below to the viscera of the head, chest, and upper part of abdomen, have its legs burnt, and be killed two or three days after, no appearances similar to those so frequently found in man, are found in any of the viscera except the bladder and rectum; but that if the cord be divided as high as the third dorsal vertebra, and the animal be similarly treated, the abdominal viscera are found in a state of congestion, with serous infiltration, and ecchymosis in different parts.

But I feel that I am wandering somewhat from the purely physiological questions to which I ought to confine myself,

and that I am trespassing on the domain of pathological inquiry. I have insensibly dwelt longer than I perhaps should have done on this subject, from its great interest, and the important practical bearings it presents.

Having thus briefly alluded to some points related to the spinal cord and the sympathetic nerve, numerous subjects at once offer themselves to me, in connexion with the encephalic portions of the nervous centres. In offering a few remarks on the present views of the functions of some of these parts, I feel somewhat relieved when I reflect that I have already occupied so much of your time, that I must rapidly bring these observations to a conclusion. I must, however, apologize for the very cursory and incomplete manner, in which I shall be compelled to treat so important a branch of my subject.

I need scarcely remind my hearers that, at the upper part of the spinal cord, and lying along the base of the brain, a series of ganglionic structures is found, completely hidden in man by the two hemispheres; but, as we trace the arrangement which exists in the various classes of animals, we find these ganglia, in some, without the superaddition of the cerebral masses. In some of the lowest vertebrata, the mere rudiments only of the hemispheres exist; and amongst the whole of the invertebrata, with but a very few exceptions, these rudiments even are entirely absent: whereas, on the other hand, as we ascend towards the higher vertebrata, we find they become more and more developed, till they attain their largest size in man. No physiologist of the present day has paid more attention to this subject than Dr. Carpenter; and the views he has been mainly instrumental in developing, with respect to the relation of the ganglia I have spoken of, to the organ of consciousness and the seat of the intelligent mind, have been most ably advanced, and have received very strong confirmation from numerous sources.

Amongst the most important points which have been advanced with respect to these encephalic ganglia, are, that they constitute not only the centres of the various organs of special sense, but of common sensation also; that they are, in fact, the seat of sensational consciousness, and the source of those consensual or instinctive actions which result from sensation, and are performed without the influence of a guiding or controlling

will. Numerous facts have been brought forward, and arguments adduced, in support of the view that the *thalami optici* constitute the centres of sensation, and the *corpora striata* the centres of instinctive or sensori-motor action.

The peripheric expansion of the grey matter of the hemispheres is connected with these ganglia by means of commissural white fibres, which, on the one hand, are supposed to convey the impressions received by the sensorial centres, upwards to the grey matter, so as to give rise to perception; and, on the other hand, to carry downwards the mandates of the will to the motor apparatus, which, obeying the injunction, brings into play the particular set of muscles required for the performance of the designed movement.

There is no principle of neurological science which has received more general assent, or is more firmly established, than that the grey matter of the nervous centres is the source of all nervous action; and, considering the hemispheres with the light of this principle, we cannot but look upon the convolutions of the cerebrum as the seat of the mental functions, the source of all psychical action and voluntary power; and the white matter as the medium by which impressions are received which, giving rise to thoughts and ideas, may eventuate in acts of the will; but when the hemispheres are absent, when there is no peripheral expansion of grey matter—no true cerebrum, in fact—mental acts do not take place, and the life of the animal is restricted to the manifestation of functions of a sensori-motor or instinctive character.

We may thus observe, from the few observations I have made, that the tendency of recent researches in the nervous system has been to separate its different portions, and to assign to each its peculiar properties and functions. There can be no doubt that the labours of Marshall Hall had a great influence in this respect, in determining the true centre of the excito-motor system; and the investigations of Dr. Carpenter and others have had a no less important effect, in throwing light on the functions of the different portions of the encephalon, in separating the psychical from the sensori-motor or consensual actions, and in assigning the seat of the former in the cerebral masses, and of the latter in the ganglionic structures I have spoken of.

There are no physiological phenomena which show more than do those of the nervous system, the importance of a wide basis of research, and the danger of hasty generalization. Had the observations of physiologists been confined to the higher forms of mammalia, to explain the phenomena witnessed in man; had they even been restricted to the vertebrata—our views of the functions of the nervous system would be much less precise and accurate than they now are. But an extensive observation of "the living experiments", to use the words of Cuvier, "which Nature has presented to us in an ascending series, in the various forms of animal existence," has demonstrated the functions of the various portions of the nervous system, and the progressive development of faculty, which attends the progressive development of structure. A careful investigation of these cases has enabled the physiologist to analyze the actions of the higher organisms, to dissect, as it were, the different portions of the nervous centres, and to apply to each its particular function. Observation seems to show that animal life is not dependent on nervous influence; that, as in the plant, nutrition, growth, and secretion, may go on without the intervention of nervous power, so, in some of the lower animals, these processes may be performed without its existence; and it has long ago been proved that, in the higher forms of animals, these acts will continue when the influence of the nervous centres is withdrawn. We cannot, therefore, look upon a nervous system as a necessary part of animal existence, but as something superadded, when definite actions are to be carried on. And we therefore find that, in those animals in which such movements are to be performed, in respondence to impressions, to sensations, or in obedience to the will, a nervous system exists, and part after part is added, or increased, in accordance with the endowments of the animal itself. An automatic apparatus is that which we first find; to it, in other animals, certain cephalic ganglia are added, which constitute, apparently, the centres of sensation, and of control over the movements of the body. But when the influence of the will is to be brought to bear on these movements, certain hemispherical masses are superadded, which are at once the organs of volition and the seat of the intellectual faculty; and a progressive increase in their development takes place, according as the animal possesses a greater power of

volition, or is endowed with intelligence and reason of a higher order.

Of the connexion of this portion of the nervous system with the intelligent mind, it would be vain for us to speculate; it is sufficient for the physiologist to know the laws by which it is governed, and the machinery by which it acts. We shall probably never ascertain the process by which an idea is conceived, or by which a thought is elaborated; as we shall probably never know the steps by which an act of the will is produced; and we can scarcely expect that our researches will demonstrate the nature of that nervous force, which has occupied so much of the attention of philosophers in all ages. Nor need we be surprised at this. What do we know of the proximate causes of gravitation, of magnetic attraction and repulsion—of the essential nature of light, of heat, of electricity? We recognise these as *forces;* and we have ascertained something of their correlation to each other; and we must be content to look upon the nervous force as one of the demonstrations of vitality—a force undoubtedly related to those to which I have alluded, but differing in its essential nature from each and all.

Interesting as must ever be, to physiological psychologists, the investigation of these questions, we, as practitioners of medicine, may safely yield them to their consideration; but, whilst we leave these subtle speculations, it becomes us to pursue the path which is already opened in the domain of cerebral physiology, and, from a wide, extensive, and careful examination of structure, to develope a more accurate knowledge of function. We know nothing of function apart from structure; we know nothing of mind apart from its material agent. To use the words of one of our most eloquent associates (Dr. Cowan, *Physiology of the Brain—Transactions of the Provincial Medical and Surgical Association*, vol. vii): "To obtain a philosophical insight into the modes of acting of our different organs, it is indispensable not only to study functions, but also the instruments of those functions; and, what is of nearly equal importance, to trace both the one and the other through every grade of organization where their presence can be recognized, to discover their essential elementary character, and to view them apart from those various combinations which obscure their manifestations in the higher

orders of created beings. . . . . We must not foolishly hope to penetrate the mystery of how mind is associated with matter. That there exists an intimate connexion between mental phenomena and organization, is perhaps most satisfactorily proved by referring to the daily and hourly experience of each individual. It might indeed with justice be asked, What do we know of mind apart from organization? Every manifestation of its presence is conveyed through material agencies; and we are not in possession of a single function by which we can perceive other than physical existences. The highest flights of creative genius fail to disrobe of a material covering the fairest beings it can fashion; and our conception of spirit is nothing more, when analysed, than indefinitely attenuated matter. The natural history of mind intimately links itself with corporeal conditions. It is as infantine as the body; it grows with its growth, and strengthens with its strength; it becomes enfeebled by age, and falters with its declining powers. There is not a vibration of the frame with which it does not sympathise, and from all antiquity bodily and mental health have been indissolubly associated. The horrors of the dyspeptic, the delicious reveries of the opium-eater, the frightful ravings of the maniac—what are they but the consequences of certain bodily conditions? And if terror may be assuaged by a narcotic, if fancy may be stimulated by the grape, if intellect may be arrested by a blow, can we for a moment hesitate as to the close union which exists between our mental acts and our bodily organization?"

The right path to the development of a scientific psychology is already entered on, and we may hope that the researches of the physiologist will pave the way for the formation of a true science of mind. Abandoning the vain speculations which characterized a former epoch, the spirit of the Baconian system has been infused into the inquiries of psychologists. To these results, the labours of some of those who are still amongst us—of Carpenter, of Noble, of Brodie, and of Dunn—have contributed; and, as a greater accumulation of facts takes place, let us hope that some subtle genius may arise, who shall unite together the scattered links of the chain of this vast inquiry, and establish the great doctrines of mind on a sound and immutable basis.

In considering the progress which neuro-physiology has made, it is impossible not to see that the present century, in this as in other departments of science, has indeed been prolific of results; nor can we contemplate the advances that have been made, or the principles and important generalizations which have been established, without experiencing feelings of satisfaction and of pride, at the distinguished part which our own countrymen have taken in the labours and researches which have led to results, so vast, and so valuable. The names of Bell, of Mayo, of Marshall Hall, of Todd, of Newport, of Carpenter, of Lockhart Clarke, are familiar to us all; and a host of other names, not less worthy of honourable mention, might be enumerated, did time permit. The works of these men have tended to place the British school of physiology in the high position it now holds; their researches have removed from the chaos of doubt and the region of uncertainty many of the most important facts of neurological science, and have given us clearer and more definite notions of the mechanism by which the automatic and instinctive actions of animal life are performed, of the mode in which sensations are perceived, and in which voluntary impulses act.

But the labours of British physiologists have not been confined to, nor do the honours to which they are entitled rest alone upon, their observations of any one particular set of vital phenomena; and whether we look to the investigation of the elementary tissues of the body, of the structure, or the functions of individual organs; whether we look to the researches into the vascular system, or the nature of the fluid which it circulates; whether we look to the examination of the anatomical characteristics of the blood, or of those subtle processes which accompany its coagulation—in fine, whether we contemplate the physiologist as investigating the nature and properties of each atomic portion of the body, or taking in, in the vast comprehensiveness of his genius, the whole range of animated nature, classifying and arranging the facts which other labourers have assisted to accumulate, deducing from them the great principles which regulate the processes of organic life, and demonstrating the unity of design, and the adaptation of means to end, which characterise the works of our beneficent Creator,—we find in each the representative and the type in the British school of physiology; we

find that each subject has had its special inquirers, each field its special labourers; and we have no fear for the onward progress of the science; no doubt that the honour reflected from our illustrious countrymen, whose discoveries have formed epochs in physiological history—from Harvey, Hunter, and Bell—will be worthily maintained by those who are now treading in the same paths, accumulating facts, and building up theories, in the same scientific departments. And I am sure I shall but express the feelings, and echo the sentiments of all, when I say with what satisfaction and pleasure we have regarded the recent effort that has been made, to do justice to the genius of one, whose name is indelibly associated, no less with the development and progress of surgery, than with the great generalizations of the science; one branch of which has formed the subject of these remarks. But, whilst we rejoice in the approaching realization of an object too long deferred, which has been earnestly desired by all the admirers of John Hunter, and by none more than by him who presides over this meeting, let us not forget that the claims of another of England's greatest physiologists, and greatest men, are yet unrecognised by any public memorial; that the remains of Harvey still lie in their obscure resting-place; and that no monument marks our admiration of his genius, or our appreciation of his labours.

But I feel, sir, that in paying the tribute of respect to the illustrious men, who have contributed so much to the advancement of physiology in our own country, I should be wanting in my duty, did I not tender our meed of praise to the distinguished labourers of continental schools, by whom the science has been so much enriched. Where so many have contributed to the progress and development of all the branches of the science, it would indeed be an invidious task to particularize names. In the course of this address, I have brought prominently before you the name and the works of one of the most distinguished of living physiologists, and I shall venture to mention another name, feeling sure that I may safely do so, in justice to the memory of him who bore it, and considering him as at once the representative and the type of the school to which he belonged.

Fifteen months ago, our science lost one of its brightest ornaments, and Germany one of her most distinguished sons.

Those who have watched the progress of physiology during any portion of the last twenty or thirty years, are familiar with the name of Johann Müller; and probably many of those present are acquainted with the most important circumstances of his life, from the perusal of an eloquent oration, delivered in honour of his memory by one of his most renowned pupils.

An ardent worker, a profound thinker, entirely devoted to the objects of his pursuit, which he followed with unabated earnestness to the period of his death, his researches and his writings have exercised a powerful influence on the progress of physiology and physiological anatomy. His views are well known in this country, from the translation of his great work by Dr. Baly; a work which forms probably a more complete handbook of the science of which it treats, than any that has been written since the time of Haller.

Müller's was not the speculative genius, which astounds us by the brilliancy of its discoveries, or the boldness of its flights; but in him we see the careful investigator, the pains-taking observer; he built up no hypotheses, unsupported by proof, and destined to fall under the scrutiny of subsequent researches; but his was a gradual working out of results, from facts which his own indefatigable labours had accumulated, and his own mind had analysed.

No one could more highly appreciate the great discoveries of science than he, and no one knew better, how few are those which change its entire aspect; and there is a passage in one of his writings which, at the same time, is so honourable to him, and so creditable to two of the illustrious men whose names I have mentioned, that I cannot refrain from quoting it. Writing in 1833, he says:—"Great discoveries have hitherto been extraordinarily rare in the history of physiological science, and, if we reckon those which have brought about a total reform in our previous notions of physiology and pathology, we shall find that two only can be regarded as of primary importance, viz., the discovery of the circulation, and of the different functions of the anterior and posterior roots of the spinal nerves; which is the greatest honour of modern times."

Gentlemen, I feel that I have trespassed too long on your time. On the important general bearings of physiology on practical medicine, it is altogether unnecessary that I should

dwell. One of the ground-works of medical science, one of the pillars which support the fabric which has been so nobly reared, we believe it will receive, year by year, fresh impulse and development; and more and more approach to that degree of perfection, to which we all desire it may attain; and we may rest assured, that in proportion as, upon the basis of structural knowledge, a correct appreciation of experimental inquiry, and a careful interpretation of pathological phenomena, we shall build up a sound physiology, and, on this, a rational pathology—so will the great principles of medical science advance—so will the diagnosis of disease become more easy, and its treatment more simple,—and so will the strongholds of empiricism be undermined, and the unsubstantial hypotheses of the day yield before the irresistible evidence of truth, and the power of demonstrated facts.

Let us, then, each pursue the path of legitimate inquiry, and endeavour to contribute our quota to the general stock of knowledge; bearing in mind the labours of those who have worked before us, and are working around us; and, although we may not hope to attain to their eminence, let us, at any rate, strive to imitate their example.

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